circumstances. So when we add further constraints to the theory of rational comparative belief and preference, as Joyce suggests we do, we succeed in *characterizing* the perspective of the evidentialist or causalist, but we do not succeed in *rationalizing* or justifying them. Ironically, in view of his rejection of Pragmatism, justification of causal decision theory can only be obtained by a further constraint on preferences for actions – one that forces a rational agent to prefer ones with higher causal expected utility rather than those with higher news-value. It remains an open question as to whether this can be done or not.

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Models as Mediators. Perspectives on Natural and Social Science, MARY S. MORGAN and MARGARET MORRISON (eds.). Cambridge University Press, 1999, xi + 401 pages.

Once upon a time, philosophers of science thought it was their business to provide a theory of scientific theories. What came to be known as 'the received view' identified theories with languages. In particular, it took it that the language of first-order logic provided the framework in which the syntactic structure of a theory (conceived as an axiomatic system) could be cast. Issues of interpretation (what is a theory a theory of?) were mostly relegated to finding the right correspondence rules which link the language of theory to the world (and especially to the relevant empirical phenomena). But soon, this orthodoxy was replaced by another, one reason for the replacement being that the 'received view' failed to explain adequately how theories hook onto the world. Semantics and set-theory (i.e., mathematics) took centre stage in the new characterization of theories. Theories were no longer identified with languages, but instead with a (class of) set-theoretic structures or, more informally, models. Where Rudolf Carnap, for instance, took models to have no more than an aesthetic or didactic or, at best, a heuristic value, the new orthodoxy – exemplified in the writings of Patrick Suppes, Fred Suppe, Bas van Fraassen and Ronald Giere – saw models as the fundamental unit of scientific theorizing, theories themselves being families of models. However, both the old and the new orthodoxy were in essential agreement on the legitimacy of the philosophical project of offering a theory of theories. Both engaged in a kind of rational reconstruction of actual scientific theories, though the new orthodoxy – also known as 'the

semantic view of theories' – insisted that theirs was in closer contact with actual scientific practice. But, by focusing on models, the new orthodoxy claimed to be in a better position than the old orthodoxy to explain how theories get applied to the world and how they represent aspects of it. Yet, the story cannot end with the fortunate clause 'and they lived happily ever after'. For they did not, and the book reviewed here shows one major reason why.

Models as Mediators, (henceforth *MaM*), has the same interest in models as the Semantic approach. In fact, it is even more enthusiastic about the ineliminable and central role of models in scientific theorizing. But it is also more pessimistic (and certainly more realistic) about the prospects of a general and ubiquitous characterization of models. Where the semantic view treated (more or less) all models as models of a theory (theoretical models), *MaM* emphasizes the diversity of models, their (partial) independence from theory and the plurality of the ways in which they can represent whatever they do. In fact, one can argue that although *MaM* inflates the role of models in science, at the same time it deflates the need to offer a general philosophical theory of models. The slogan that is encapsulated in the title of the book is, in a sense, the most informative *general* statement about models and their role that *MaM* offers. The book stems from what Nancy Cartwright (p. 241) calls 'the LSE/Amsterdam/Berlin modelling project' and consists in a number of detailed case studies in physics, chemistry and economics which aim to illustrate (if not prove) the slogan that 'models mediate between theory and the world' (p. 242). The choice of focusing on different sciences is not accidental, and creates an impression of diversity and unity at the same time. On the one hand, there seems to be no substantive feature shared by all the models discussed in this volume. On the other, *MaM* purports to show not only that models are central to both the social and the natural sciences, but also that models function in (more or less) the same autonomous way in both domains.

MaM is a collection of papers written by different authors. Four of them are about economics, five about physics and one about chemistry. Since most of them discuss in some detail actual models, they tend to be rather technical and the readers who are not familiar with either physics or economics will find the task of following the details quite difficult. Yet patience with the book is rewarded, since most chapters have important methodological and philosophical insights and arguments. The heterogeneity of the chapters is counterbalanced by the important leading chapter by Margaret Morrison and Mary Morgan. Morrison and Morgan's paper does two things. First, it outlines 'an account of models as *autonomous* agents' and sketches 'how they function as *instruments* of investigation' (p. 10). Second, it locates all other pieces in the volume *vis-à-vis* this account. So, the prospective reader is recommended to start

with the leading piece and then, according to her interests, to move through the rest. Reading this chapter creates the firm impression that all the essays in the volume share some background views in common and engage in the same project. The reader, however, should not be misled into thinking that there is a 'Mediators' theory of theories. The authors do not seem to share any substantial thesis about the nature of scientific models, and the contributions turn out to be less homogeneous than one would expect them to be after reading the leading piece.

One central issue that crops up right at the beginning is the relation between models and theories. *MaM* presupposes throughout that there are theories and models and the world. Leaving the world aside, *MaM* presupposes that there is a distinction between theories and models. We are told that 'we should be mindful of the ways that models and theories do interact' (p. 8). We are also told that models are (partially) independent from theories, that models may represent 'some aspect of our theories about the world' (p. 11), that models are situated 'outside the theory-world axis' (p. 18) and suchlike. But we are not told what theories are and how they are different from models. This omission is revealed in various chapters. Both Ursula Klein and Mary Morgan, for example, rely on an intuitive distinction between different levels of concreteness, labelling what lies at the most abstract end of the scale 'theory', and what lies towards the other end 'models'. However, nowhere in the book is such a distinction fully articulated and made explicit. The scientists' 'rough and ready distinction' (p. 18, fn 3), which is briefly mentioned in the piece by Morrison and Morgan, seems to be at odds with what most of the individual authors have to say on models. Most of them reject (implicitly or explicitly) the 'rough and ready' view that, relative to theories, models are 'less certain or incomplete in important respects' (*ibid*.). On the contrary, the models discussed in this book seem to be more complete than theories (because they are endowed with more concrete details), and more certain than theories (because it is primarily the models, as opposed to the high-level principles, that are confirmed by the empirical evidence). We think that the problem we raise here is not a mere quibble. Its investigation would help demarcate clearly the view of *MaM* from the Semantic conception. As things stand, it is not entirely clear whether *MaM* offers an alternative – fundamentally different – view or whether it suggests ways in which the Semantic view could be complemented. To be sure, Morrison and Morgan state, in passing, that 'theories consist of general principles that govern the behaviour of large groups of phenomena' (p. 12). But this statement is consistent with both the 'received view' and the Semantic view. In particular, it can be construed in such a way that the general principles single out precisely the theoretical models of the Semantic view.

Things get more complicated when it comes to the characterization

of models. Here Morrison and Morgan are very explicit: 'we do not see ourselves as providing a ''theory'' of models' (p. 12). This might be disappointing for all of those who expected *MaM* to address this philosophical issue. But the individual chapters make clear why *MaM* cannot possibly offer such a theory: the models dealt with in the book are so diverse and disparate that they cannot really be covered by a general description. Morrison, for instance, talks of 'theoretical models' which can be derived from theory (p. 46), of models which are not strictly theoretical, but not phenomenological either (e.g., the nuclear models), and of phenomenological models which are 'motivated solely by the phenomenology of the physics' (p. 54). According to Morrison all these models depend on theory – so she rightly dismisses the view that phenomenological models are theory-free (p. 44). Mauricio Suarez, on the other hand, focuses his attention on 'mediating models', which ' ''fill . . . in'' the abstract descriptions afforded by the theory', and contrasts them with other kinds of model (pp. 168–9). In support of this, he offers the case-study of superconductivity and concludes that the relevant model has an 'independent and non-reducible phenomenological' dimension (p. 187). The general picture that emerges is one of diversity.

This impression is accentuated by the papers which deal with economic case studies. Most of these papers simply describe models in economics, with little discussion of what models are in general. This probably reflects the relative lack of a shared paradigm in the philosophy of economics, where the Semantic view has won little consensus and where no one seems to agree on which philosophical problems (if any) are worth tackling in the first place. Geert Reuten's chapter, for instance, is mostly devoted to illustrating and trying to make sense of Marx's 'Schema of Reproduction'. Although some general methodological remarks are attached at the end of a lengthy case study, the overall impression is that this paper is mostly driven by exegetical preoccupations. So, unless one is interested in the exegesis of Marxian economics, it is not clear what to make of this detailed historical reconstruction. Similarly, Adrienne van den Bogaard's chapter is a nice piece of history of ideas, with an eye on the institutions that influenced (but were also conditioned by) the usage of different models and statistical techniques. But little philosophical elaboration can be found in this paper either.

Perhaps, a general descriptive statement about what models are that emerges from the book is that they are kinds of 'representative structure' (p. 33). But even here, we get little by way of an account of representation. R. I. G. Hughes offers a suggestive summary of his DDI account, which renders representation a function of three things: (a) the Denotation of elements of the subject of the model by elements of the model; (b) the Demonstration within the model of several conclusions; and (c) the Interpretation of these conclusion in terms of the subject of the model

(p. 125). Morrison talks of 'structural dependencies' (p. 63) but she also allows a (rare) kind of 'mirroring', where we get 'an increasingly realistic picture of the actual object or physical system' (p. 60). Yet, her general point is that 'there is no one way to characterise the nature of . . . representation' (p. 64). Suarez takes issue with the view that representation proceeds via 'deidealisations of theory' (p. 182). Cartwright dismisses the notion of representation as 'picturing' and suggests that models 'resemble the situation they represent' (p. 262). Finally, Stephan Hartmann connects representation to a loose 'story' that accompanies the interpreted formalism of the model (p. 344). Morrison and Morgan sum it all up by saying that 'a representation is seen as a kind of rendering' (p. 27).

If all these sound like weaknesses in the book, it also has its strengths which compensate for them. *MaM* might not offer a theory of what models are, but it does offer a kind of theory of *what* models *do* and *how* they *do* it: models mediate between theory and the world and they do that by being autonomous agents, that are irreducible to either theory or the data. Now, insofar as the Semantic View denies the autonomy of the models (an issue on which, we think, the jury is still out), *MaM* offers a substantially different approach. This approach is broadly *functionalist*. Models are not individuated by their content, nor by any account of how they represent. Rather, they are individuated – and distinguished from theories – by the *functions* they perform, of which there are four. The first relates to how they are constructed: seldom does the theory provide the entire stock of building blocks for the model; in most typical cases, elements from one or more theories, other models, and the data cooperate to build a model. Models are 'autonomous' from each one of these sources, in the sense that they are not derived uniquely from any one in particular. The second function relates to their being used as instruments for the exploration and development of theory as well as, more directly, for more accurate measurements. The third relates to their *ability* to represent. And the fourth function relates to their ability to enhance learning: this is not exhausted in the construction of the model; it is supplemented by the use of the model.

The foregoing functionalist conception is best seen as emerging from a set of broad generalizations about the way models function, their role, their relation to theory and empirical evidence, the way they are used, and the sort of knowledge they embody and can generate. Such generalizations are supposed to be derived inductively from the case studies in this book. An especially suggestive way in which models function is proposed by Cartwright. She notes that most concepts of high-level theories are abstract and that models – what she calls 'interpretative models' (p. 257) – are indispensable in giving concrete content to them. In fact, her thesis seems stronger than that, since she notes that ''Force''

. . . being abstract, it can only *exist* in particular mechanical models' (p. 257, emphasis added). Cartwright's paper also provides the most direct attack on the Semantic view, which is criticized as a specific instantiation of what she calls the 'vending machine' view of modelling. According to this view, the model is already 'in' the theory, and the scientist's job is reduced to the (non-trivial) task of choosing the best machine or theory that is able to generate an appropriate model. Cartwright's own example from superconductivity, as well as other case studies in this volume, show convincingly that modelling is much more complicated than that. Her chapter puts to work the idea, also highlighted in Marcel Boumans's chapter, that modelling is a creative enterprise. Boumans introduces and illustrates two theses that are echoed at several other stages later in the volume. The first claim is the heterogeneity of elements that make up a model. The second thesis is that the ways in which the various ingredients are put together vary from case to case, and follow no general rule: there is no general recipe for model building. However, a proponent of the Semantic view could always reply to Boumans and Cartwright that irrespective of how models are actually built, each model will always be 'in the theory' because the theory simply *is* the set of (highly theoretical, interpretative and representative) models that make it up. We shall leave it to the reader to decide whether the Semantic view should be praised for its generality, or – as the authors in *MaM* seem to suggest – criticized precisely on the grounds that this alleged generality fails to distinguish between different kinds of models and the ways in which they are created.

In any case, the proposed functionalist account of models is original enough to be an important new contribution to the subject. It would have been better if the individual chapters had instantiated this account in a more coherent and systematic way. But even as they stand, each case study highlights some aspect(s) of this account. A remaining worry, however, relates to the philosophical implications of this account. It is one thing to describe how models function and to explain this function by means of their autonomy; and it is quite another thing to engage in the philosophical issue of how models are vehicles for substantive knowledge of the world. To be sure, some of the papers in the volume (e.g., the papers by Morrison, Hughes, Suarez and Cartwright) do deal with this philosophical issue. But there does not seem to be an informative, overall approach. In fact, there are conflicting views. For instance, Morrison downplays the distinction between theoretical and phenomenological models and argues that it is orthogonal to the issue of how realistic the representation of the model is (p. 63). She nonetheless stresses that models can offer substantive theoretical knowledge of the world. Cartwright offers a sophisticated view of how theories relate to

the world, one that makes the models carry the proper ontological commitments of theories, but stresses that this view is consistent with the thesis that theories are 'warranted by their empirical successes' (p. 259). But Suarez seems to differ. After introducing a distinction between 'degree of confirmation' and 'degree of confidence', he offers an anti-realist gloss of the connection between theories and models. Unlike Morrison (and perhaps Cartwright), he seems to restrict the knowledge that models offer to 'the phenomena' (p. 195). Or take Boumans. Although he takes it to be the case that models have 'in-built' justification, he never raises the issue of whether this is genuine justification at all. He claims that his exemplary models 'were satisfactory to the model builders'(p. 95), without stepping up to the normative level. Klein and Morgan focus on learning from experimentation and manipulation of material or quasi-material models. One of the models examined by Morgan provides information concerning what policy makers should know and what they should be able to control in order for some intervention to be feasible and effective. Reuten's discussion of Marx's 'knife-edge' caricature similarly stresses the counterfactual (and mostly negative) nature of the knowledge provided by economic models. This diversity might seem natural. After all, one would expect that a functionalist account of models should be neutral about what kind of knowledge models offer. But since it is part of the functionalist account that 'models are both a means to and a source of knowledge' (p. 35), one is led to expect some general account of what sort of knowledge this is. Even if *MaM* aims to show how different models provide different kinds of knowledge in a truly pluralist vein, one might have expected some unity behind this pluralism.

This tendency to programmatically elude standard philosophical questions is sometimes frustrating, but in spite of that (or perhaps *by virtue* of it) the project undertaken in *MaM* is highly interesting and suggestive. The case studies are as detailed and realistic as those in the best relevant work in the philosophy of science. It opens up new ways of thinking about models and their relation to theories, and promises that further relevant work will cast more light on the central philosophical issues on which the project focuses. There is no doubt that all those who think about (or work with) models will learn a great deal from the book.

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